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the end destroy each other and so vanish. By developing the disturbing function in a general form it is possible, by an application of the method of the variation of arbitrary constants, to ascertain what terms destroy each other without its being necessary to determine the value of the terms in question. Thus if R be a function of the form $r \, {}^{\sin}_{\cos} \, \beta$, where r is constant and only β variable, as far as constant terms the function

$$R \int R \, dt = 0.$$

It can be further shown that in this expression every term is multiplied by the coefficient of the time in its argument. By similar considerations to these the work can be much reduced.

To show that the class of terms which have been referred to vanish to the order m^{10}, it would be necessary to show that they are mutually destructive to the fifth power of the disturbing forces, whereas no general development of the theory of the variation of constants seems to have been pushed beyond the terms of the second power of the disturbing forces. By a transformation I have pushed the development as far as the third power of the disturbing forces, and in part, though at present not completely, to the fourth power.

By this means it can be shown that the terms of the order m^4 and m^5 vanish from the perturbations in the mean motion, or before integration are multiplied by the small coefficient of the time in their argument. The same is true of most if not all of the terms arising from the second power of the disturbing force of the Sun and of the order m^6 and upwards. The same does not appear to hold of the third power of the disturbing force of the Sun or of the fourth power of the entire disturbing forces. I have not yet completed this last portion, but terms of the order m^6 appear to actually remain undestroyed. This result is confirmatory of the results obtained by direct computation in a very different manner.

If, then, there remain terms of the order m^6, it is quite possible that Hansen's theoretical values of the two terms of long period arising from *Venus* may be accurate.

On the Telescopic Observations of the Transit of Venus 1874, *made in the Expedition of the British Government, and on the Conclusions to be deduced from those Observations.* By E. T. Stone, M.A., F.R.S., Her Majesty's Astronomer, Cape of Good Hope.

I received by the last mail a copy of the Parliamentary Report whose title heads this Paper. This report contains the details of the observations of contact made by the observers of the British

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Expeditions and the deduction of a value of the mean solar parallax (8″·76). I have examined the observations with great care, and discussed them in accordance with my own views. The result to which I have been led differs most materially from the value 8″·76 obtained in the Report, and I have therefore felt it my duty to lay my views before astronomers for their consideration. I presume that, with the evidence of these British observations, as well as that of the French observations before us, we need no longer insist upon the fact that the contacts in a transit of *Venus* are not mere geometrical contacts, but that when well seen in a fairly illuminated field, and when attention is directed to the point, the connection between the apparent limbs of *Venus* and the Sun is formed and broken gradually. The exact name which different observers may please to give to the phenomena observed matters but little, provided we can identify the observations which have taken place with the same angular separation of the limbs. The choice of assumed difference of semi-diameters applicable to the observed contacts is, as I have always maintained, the one difficulty in obtaining a very accurate value of the solar parallax from discussions of contact observations of the limbs of *Venus* and the Sun, and this same difficulty is presented in the methods of Halley and Delisle. The whole strength of the method for the determination of the solar parallax rests upon the assumption that there are phases of the contact which can be recognised by the observers as distinctive, and which at all the stations take place with the same angular separation of the limbs. It is not of course assumed that this is absolutely true; but the accuracy with which this assumption can be made in any class of observations, under the actual circumstances of observation, combined with the magnitudes of the parallax-factors, alone limits the accuracy attainable in the deduced value of the solar parallax. It is useless, therefore, except as a check on the principal phase, to record times corresponding to any but well-marked phases; and all time records corresponding to "limbs well separated," "bright light between the limbs," and such like, except as an indication of the time of last appearance of some disturbance previously seen, are without value for our present purpose.

I see no reason for abandoning my old views that the two phases of greatest value are the so-called apparent contacts and the so-called real contacts, of which the latter is the more important. By the real contact I merely mean the phase at which the observer first perceives the formation or disappearance of any permanent connection between the limbs of *Venus* and the Sun.

When we come to examine the observations made in 1874 we find that the apparent contacts have not been observed at the Sandwich Islands, one of the principal stations. It would almost appear from Mr. Nichol's remarks, and those of Capt. Tupman, that the measures of the cusps were run too close to the time of

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contact to allow of any successful attempt to observe the apparent contacts. I am therefore compelled to restrict myself entirely in the present Paper to the real contact; but I shall divide them into two classes: the first corresponding to a disturbance of a very slight character, which can only, in my opinion, be seen under the conditions of a bright field and fair definition; and the second and more important phase when this shadow, or black drop, or haze, first assumes, or last appears of, a well-marked and permanent character. It will be seen, from the few attempts at the first phase, that the change of distance of centres is sufficiently great to prevent any confusion arising between these two kinds of observations, and the principal phase is well separated from the apparent contacts.

Sandwich Islands.

The following is Mr. Johnson's account of his observation at Waimea:—

	h	m	s	
{Cusps Indefinite light} beginning to appear to meet .	7	17	57·8	G.S.T.
Meeting of cusps appeared to take place, and light clearing. I am inclined to think the actual contact took place between $7^{h}\ 17^{m}\ 57^{s}\cdot 8$ and $7^{h}\ 18^{m}\ 7^{s}\cdot 1$	7	18	7·1	
A distinct band of light between edge of *Venus* and the Sun's limb	7	18	12·0	

I have taken this observation under consideration, first, because it is preceded and followed by remarks showing that the time of total disappearance of the connection could not differ much from the time recorded, viz. $7^{h}\ 18^{m}\ 7^{s}\cdot 1$. It is, however, probable that a mean between the two observations, $7^{h}\ 17^{m}\ 57^{s}\cdot 8$ and $7^{h}\ 18^{m}\ 7^{s}\cdot 1$, might be the true time of disappearance of the ligament.

Next, referring to the Honolulu observations, we have Capt. Tupman's observation:—

	h	m	s	
There was no *black* drop nor ligament. The planet was perfectly circular, and nothing whatever disturbed the sharpness of its outline at the place of contact. The band of light gradually brightened, and as Mr. Clapp said "twenty" it was sufficiently bright to induce me to consider the contact as established. My impression *afterwards* was that even at this time the Sun's limb was not yet perfectly distinct, and that some shadow remained on the light, but nothing was recorded after the "twenty." At that time the general appearance was similar to the model a second or two after contact	7	17	59·8	G.S.T.

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Lieut. Noble's observation:—

	h	m	s	
There was no black drop, no ligament, but a rough dark shade, which gradually faded off to a thin tint corresponding to the phenomena I had observed in the model. This, instead of being nearly instantaneous, as the model generally showed, extended over some 20 seconds. The thin tint is that recorded, and I consider this the time of contact. I am certain it could not have been later than this	7	18	0·2	G.S.T.
Capt. Cator's observation (with diagram)	7	18	2·2	

Lieut. Noble also states that at $7^{h}\ 18^{m}\ 20^{s}{\cdot}2$ the contact was long passed.

Now, we have here four observations of the real contacts. They differ amongst themselves rather more than could be wished, but not more, I believe, than such observations must fairly be expected to differ. There would be a better appearance of agreement if we took for Johnson's time the mean as before mentioned; but it appears to me better to take the results as they stand, and we have for real contact at the Sandwich Islands—

	Greenwich Sidereal Time. h	m	s	Local Tabular Distance of Centres. ′	″
Capt. Tupman	7	17	59·8	15	37·58
Lieut. Noble	7	18	0·2	15	37·57
Mr. Johnson	7	18	7·1	15	37·35
Capt. Cator	7	18	2·2	15	37·40
Mean	7	18	2·3	15	37·48

Then, if $x =$ a constant for Ingress observations of real contact, $\delta\pi =$ connection to the assumed parallax $8''{\cdot}95$, and $\delta\pi = 8{\cdot}95 \times \frac{y}{100}$, we have for the real contact equation

$$x + 0{\cdot}2280\,y = 15'\ 37''{\cdot}48 \qquad \text{4 observations.}$$

It will be seen that the discordance in the estimation of the contact does not differ for any observer more than $0''{\cdot}13$ from the mean, a small quantity to be measured on the Sun's edge.

Rodriguez.

The real contact observations made by Commander Wharton and Lieut. Neate at this station are both clearly marked as the time of last appearance of haze or ligament.

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Commander Wharton says :—

	h	m	s	
Directly after, a blackish haze was discernible joining the planet and the continuation of the Sun's limb; but from the agitation of the limb (the morning being cloudy) this haze was not distinctly defined and did not seem to change colour before disappearance. The moment of total disappearance of haze was tolerably defined, but I could not be certain to two or three seconds. The band of haze did not narrow at all before it broke that I could see. The time recorded is of total disappearance of haze as nearly as could be guessed	7	39	31·0	G.S.T.

Lieut. Neate :—

The ligament then gradually changed in colour (but not in breadth) getting lighter till it disappeared apparently suddenly, when the light between the limbs was uninterrupted . . .	7	39	27·2

Lieut. Hoggan :—

The ligament appeared to be decreasing in width very perceptibly	7	39	33·1
It had faded away, and light appeared all round the planet's disk	7	39	48·6

The first recorded time is early, the last would appear, from the use of the past tense, too late. A mean time, or $7^{\mathrm{h}}\ 39^{\mathrm{m}}\ 41^{\mathrm{s}}\!\cdot\!85$, cannot be much in error and appears preferable to the use of either of these times. I may mention that, had I used the earlier time as that of last appearance, it would have given me a very slightly larger value of the solar parallax. Thus we have for real contacts at this station :—

	Greenwich Sidereal Time. h	m	s	Local Tabular Dist. of Centres. ′	″
Com. Wharton	7	39	31·0	15	37·88
Lieut. Neate	7	39	27·2	15	38·00
Lieut. Hoggan	7	39	41·9	15	37·58
Mean	7	39	33·4	15	37·82

Real contact equation $x - 0{\cdot}1947\,y = 15'\ 37''{\cdot}82$ 3 obs.

At Kerguelen we have observations of the disappearance of the connection or real contacts by three observers.

Lieut. Corbet :—

	h	m	s	
At this moment there was no more shade between the two limbs; it disappeared	7	40	33·2	G.S.T.

Lieut. Coke :—

The film disappeared	7	40	49·6

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Lieut. Goodridge:—

	h	m	s	
All appearance of shadow totally disappeared .	7	40	43·6	G.S.T.

Lieut. Corbet's time appears decidedly too early; but, on account of the difficulty of such observations, it does not appear desirable to assume too small a probable error, and I take the mean. It will be seen on consideration that if I had given less weight to Lieut. Corbet's observation, the value of the solar parallax deduced would have been greater. We have, then, for this station:—

	Greenwich Sidereal Time. h m s	Local Tabular Dist. of Centres. ′ ″
Lieut. Corbet	7 40 33·2	15 38·29
Lieut. Coke	7 40 49·6	15 37·81
Lieut. Goodridge	7 40 43·6	15 37·93
Mean	7 40 42·1	15 38·01

and the equation of real contacts is

$$x - 0·2150y = 15' 38''·01. \qquad 3 \text{ obs.}$$

There remains the ingress observed by Major Palmer at Burnham. He says:—

	h	m	s	
At . . . . as far as could be judged, the ligament seemed to undergo a change in depth of colour; but clouds prevented me from seeing whether any streak of light connecting cusps played across it.	7	29	38·1	G.S.T.
My last sight of the planet was at . . . . . at which moment I *thought* I saw a very slight wavering streak of light, *though the cusps* (now very close) *had not fairly met.*	7	29	41·3	
I judged that it wanted about five seconds to complete separation of limbs. I counted the clock ticks for five seconds, and gave a signal for an estimated contact at	7	29	46·3	
The Sun did not appear again until about . .	7	43		

On account of cloud the absolute disappearance of the ligament at this station is only an estimation, but it was not gone at $7^h\ 29^m\ 41^s·3$, which is important. Major Palmer's estimate places its disappearance at 5^s later, but such an estimation made during a glimpse through cloud could only be a rough one. I wish, however, to show that I am not straining the evidence in favour of any particular value of the solar parallax, and I will accept it as an observation, and work up the Ingress observations both including and excluding it.

The real contact equation derived from this observation is

$$x - 0·0140\, y = 15'·38''·07.$$

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It will be seen that, on account of the small factor of y, this observation is not of much value for the determination of the solar parallax.

First, excluding the Burnham observation, the three equations for Ingress are

$$\sqrt{4}\,(x+0{\cdot}2280\,y) = \sqrt{4}\times 0{\cdot}48,$$
$$\sqrt{3}\,(x-0{\cdot}1940\,y) = \sqrt{3}\times 0{\cdot}82,$$
$$\sqrt{3}\,(x-0{\cdot}2150\,y) = \sqrt{3}\times 1{\cdot}01,$$

which lead to

$$x-0{\cdot}0315\,y = +0{\cdot}741,$$
$$x-1{\cdot}4586\,y = +2{\cdot}193\,;$$

whence

$$x = +0''{\cdot}71,\ y = -1{\cdot}017, \text{ and solar parallax} = 8''{\cdot}859.$$

These values give the following residual errors :—

Obs.—Comp.	
$+0''{\cdot}00$	Sandwich Islands.
$-0{\cdot}09$	Rodriguez.
$+0{\cdot}08$	Kerguelen.

Next, including the Burnham observation, we have, to find x and y, the equations

$$x-0{\cdot}0299\,y = +0{\cdot}771,$$
$$x-1{\cdot}3971\,y = +2{\cdot}145\,;$$

whence

$$x = +0''{\cdot}741,\ y = -1{\cdot}005, \text{ and solar parallax} = 8''{\cdot}860.$$

The residual errors are now

$-0''{\cdot}03$	Sandwich Islands.
$+0{\cdot}31$	Burnham.
$-0{\cdot}12$	Rodriguez.
$+0{\cdot}05$	Kerguelen.

It would appear that Major Palmer's estimation is about 10^{s} too early, a result which can surprise no one. The general result from the Ingress observations is therefore

$$\pi = 8''{\cdot}860 \pm 0{\cdot}136 \times e,$$

where e is the probable error estimated in arc between the difference of the tabular distance of the centres corresponding to the time given by an observer and that which corresponds to the mean phase adopted at all the stations for Ingress. I

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think *e* is certainly not greater than $0''\cdot3$, and that the probable error of the above result cannot exceed $0''\cdot04$.

Anyone who will examine my work will find that I have not selected the observations with a view to securing the largest possible value of the solar parallax. I have boldly taken the mean results of all attempts to observe the same phase, and I have done this on principle, although the observations may appear to differ somewhat wildly amongst themselves. For instance, I have taken for the time of the last appearance of the ligament at Kerguelen's Island the mean of the three results by Corbet, Coke, and Goodridge, although Corbet's time is 16^s earlier than that given by Coke; but in this transit of 1874 the velocity of separation of the limbs was so small that an error of 16^s is equivalent to less than an error of half a second of arc in the separation of the limbs, and this is certainly not an impossible quantity for the disagreement of two measures made upon the Sun's limb. As we have, however, the positive evidence of two observers that some connection between the limbs did exist for more than 10^s after the time given by Corbet, we might no doubt legitimately assume that all connection between the limbs of *Venus* and the Sun had not ceased at the time indicated by Corbet; but in my opinion we could not therefore legitimately reject his observation. It must be remembered that when we attempt to draw a value of the solar parallax from contact observations, we have to make an assumption that all attempts to observe the same phase are made with the same angular separation of the limbs; and it is certainly more probable that such an assumption will be satisfied when mean values are adopted rather than extreme ones. The discordances which exist amongst the observers at the same station are independent of any assumed value of parallax, and are therefore valuable as an indication of the errors which may be expected when we assume that the contacts for all stations take place with a mean distance between the centres; and we cannot fairly expect the discordances which arise from this assumption to be quite as small as those which exist among observations of the same phase made at a single station. I have been guided by this consideration in the rather liberal allowance which I have made for the probable error of the result.

The only cases in which I consider that I have exercised any arbitrary selection of the materials are:—

(1) The above case of Corbet (I might have given the observation less weight, or rejected it);

(2) The case of Johnson (I might have taken the mean value as before indicated);

(3) The case of Hoggan (in which I have taken a mean value).

But none of the alternatives to the selection which I have adopted can change the value of the solar parallax more than

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0″·02, and I believe that I have had good reasons for the selections made.

It will of course be easy for anyone to vary the solution within small limits by slight changes such as those indicated, but I believe that no important changes in the value 8″·86 is possible without some clearly erroneous selection of a phase attempted to be caught by the observer. In the original solution contained in the Report an assumption has been made that what I have taken to be real contacts at the Sandwich Islands are some phase sensibly preceding the total disappearance of the connection between the limbs of *Venus* and the Sun; whilst some remarks made by the observers at different intervals after the total disappearance of any connection are supposed to be the real contact observations, and are marked "phase γ." These remarks, I have no doubt, were merely made by the observers to check the reality of the disappearance of any connection; and as they have no distinct relation to a definite separation of the limbs, they are only useful as checks upon the principal phase. It is this mistake which is the principal and almost sole cause of the difference between the value of the solar parallax obtained in the Parliamentary Report and that which I have obtained from a discussion of these Ingress observations. Before passing to the Egress observations, I would remark that the observations at Ingress and Egress are of a somewhat different character. At Ingress the observers are carefully watching the disappearance of something distinctly seen; whilst at egress they have to pick up, sometimes even through clouds, the first clear indications of a disturbance of the Sun's limb near the point of contact. There is therefore no great certainty, nor is there any necessity, that the phases at Ingress and Egress should exactly correspond. I fear, moreover, that attempts to estimate the degree of blackness of the ligament at Egress have led some of the observers to wait rather late before they have considered the contact as fairly established, and I doubt the expediency of these attempts to subdivide the phases.

The value of the Egress observations contained in the Parliamentary Report is also much diminished through the comparative failure, on account of cloud, of the observations at Kerguelen's Island; but the observations clearly point to a larger value of the solar parallax than that which I have deduced from the Ingress observations.

Mokattam and Suez.

We have four observations of the ligament well marked:—

Captain Browne says:—

> The shadow began to appear, and I tried to note the instant, but feared to interrupt my wife counting so near contact. Tried to note some

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one instant, but could not, *the shadow was so gradual.* The Sun getting brighter, I saw a modified short ligament form, but did not see the edges form into sharp curves and creep apart, as sometimes in the model. Blackness gathered gradually, and at last came an instant when it seemed to get as black as the planet, and to have no light crossing it, which I noted by repeating the seconds then called by my wife . . . $11^{h}\ 17^{m}\ 17^{s}{\cdot}8$ G.S.T

Mr. Hunter :—

Ligament now well marked . . . . . . $11\ 17\ 24{\cdot}08$

At this station, then, we have the following observations :—

	Greenwich Sidereal Times. h m s	Local Tabular Dist. of Centres. ′ ″
Mr. Newton	11 17 14·3	15 41·34
Miss Newton	11 17 16·6	15 41·41
Capt. Browne	11 17 17·8	15 41·45
Mr. Hunter	11 17 24·1	15 41·62
Mean	11 17 18·2	15 41·455

The equation for this station for *ligament well marked* is

$$x + 0{\cdot}2020\,y = 15'\,41''{\cdot}455 \qquad 4 \text{ obs.}$$

There is also a very early observation of a fine line at $11^{h}\ 16^{m}\ 39^{s}{\cdot}1$ with a tabular distance of centres equal to $15'\ 40''{\cdot}08$, and two geometrical contacts which correspond to a tabular distance $15'\ 44''{\cdot}54$ of the centres. It will be seen that the principal phase is quite separated from the two other phases, and that no confusion need be feared from the use of one for the other, except for the observations at Kerguelen, which were made through clouds. The observers, however, appear to have waited until the ligament was very strongly developed.

Thebes.

There is a little difficulty in interpreting the observations at this station, from the language of the observers; but the following three so-called "contacts" must be the same phase as that observed at Mokattam and Suez. This appears not only from the value of the tabular distances of centres, but also from the kind of contact represented in Colonel Campbell's figure :—

	Greenwich Sidereal Times. h m s	Local Tabular Dist. of Centres. ′ ″
Admiral Ommaney	11 16 46·8	15 41·43
Mrs. Campbell	11 16 51·0	15 41·56
Col. Campbell	11 16 53·3	15 41·64

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There is an early observation of Mrs. Campbell's at $11^h\ 15^m\ 44^s.3$, corresponding to a tabular distance $15'\ 39''.31$ and called an appearance of shadow, and also one at $11^h\ 16^m\ 29^s.5$, with a tabular distance $15'\ 40''.84$ and called a small black drop. The equation for this station for ligament very well marked is

$$x+0.1910y = 15'\ 41''.543 \qquad 3 \text{ obs.}$$

Rodriguez.

We have here two times recorded near the real contact so close together that some doubt may exist as to which should be taken. I will exhibit the results on both suppositions, and it will be found that the resulting values of the solar parallax do not sensibly differ.

Lieut. Neate :—

	h	m	s	
The first phenomenon noticed was the formation of a brown haze between the two limbs, and in spite of the excessive boiling this formation appeared instantaneously, and in appearance it closely resembled what I had seen at ingress in the second noted phenomenon	11	8	39·7	G.S.T.

Lieut. Hoggan :—

	h	m	s
The light was not entirely interrupted at . . .	11	8	41·4
Light no longer visible between the limbs of the Sun and *Venus*, being interrupted by a band of about one-eighth of the diameter, and the same degree of blackness as the planet	11	8	46·4

Commander Wharton :—

	h	m	s
The brown haze appeared quickly, though not well defined at its edges as a ligament; it was of considerable breadth. Time recorded is of first appearance	11	8	41·7
The haze or ligament darkened perceptibly. It is recorded as first appearance of a black hazy drop, the time of which was not very distinctly marked	11	8	50·3

There are also observations of a very early phase by Lieut. Hoggan, at $11^h\ 8^m\ 8^s.5$ G.S.T., of a slight distortion of the planet's limb which did not remain steady, corresponding to a tabular distance $15'\ 40''.30$, and three apparent contacts corresponding to a mean distance $15'\ 43''.307$. Our observations are then :—

1st Set.	Greenwich Sidereal Time. h	m	s	Local Tabular Dist. of Centres. ′	″
Lieut. Neate	11	8	39·7	15	41·27
Lieut. Hoggan	11	8	41·4	15	41·37
Comm. Wharton	11	8	41·7	15	41·34
Mean				15	41·327

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and the equation for this phase is

$$x + 0{\cdot}0270\,y = 15'\,41''{\cdot}327 \qquad 3 \text{ obs.}$$

2nd Set.	h	m	s	′	″
Lieut. Hoggan	11	8	46·4	15	41·53
Com. Wharton	11	8	50·3	15	41·62
Mean				15	41·575

and the equation for ligament strongly marked is

$$x + 0{\cdot}0270\,y = 15'\,41''{\cdot}575 \qquad 2 \text{ obs.}$$

Kerguelen's Island.

Rev. W. Sidgreaves:—

	h	m	s	
Sun nearly obscured. Circular contact. Nothing seen before this. Very uncertain observation; image scarcely visible through the cloud . .	11	1	58·1	G.S.T.

Rev. S. J. Perry:—

A good geometrical contact. There was no appearance of black drop before contact, but this might have been due to the obscuring clouds .	11	2	15·8

Lieut. Goodridge:—

Passing clouds. At this time a few lines or slight shadow appeared between the two limbs, just visible through cloud	11	1	39·9
At this time the shadow gradually darkened until the black drop assumed a solid appearance of the same density as the planet. A dark cloud came over, and I lost all further view . .	11	1	53·4

We have, then, the following observations at this station:—

	Greenwich Sidereal Times. h	m	s	Local Tabular Dist. of Centres. ′	″	
Lieut. Goodridge	11	1	39·9	15	40·61	Shadow.
Lieut. Goodridge	11	1	53·4	15	41·03	Solid black drop.
Rev. W. Sidgreaves	11	1	58·1	15	41·14	Contact.
Rev. S. J. Perry	11	2	15·8	15	41·68	Contact.

Whatever interpretation we may be pleased to put upon these observations, there is no doubt that a well-marked connection was established at $11^{h}\,1^{m}\,55^{s}{\cdot}7$ G.S.T. This is shown by both Sidgreave's and Goodridge's observations. It is impossible to believe that the time given can be much too early, and therefore

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the corresponding distance of centres 15′ 41″·08 cannot be much too small.

I will exhibit the values of the solar parallax which would be required to satisfy the equations. (1) Taking the first set of observations at Rodriguez, we have the following equations:—

$$\sqrt{4}\,(x+0{\cdot}2020\,y) = \sqrt{4}\times 0{\cdot}455,$$
$$\sqrt{3}\,(x+0{\cdot}1910\,y) = \sqrt{3}\times 0{\cdot}543,$$
$$\sqrt{3}\,(x+0{\cdot}0270\,y) = \sqrt{3}\times 0{\cdot}327,$$
$$\sqrt{2}\,(x-0{\cdot}1000\,y) = \sqrt{2}\times 0{\cdot}080.$$

These lead to the following equations for the determination of x and y:—

$$x+0{\cdot}1052\,y = 0{\cdot}3825,$$
$$x+0{\cdot}2336\,y = 0{\cdot}5461\,;$$

whence

$$x = 0''{\cdot}249,\ y = +1{\cdot}274, \text{ and parallax} = 9''{\cdot}06.$$

(2) Taking the second set at Rodriguez, we have

$$\sqrt{4}\,(x+0{\cdot}2020\,y) = \sqrt{4}\times 0{\cdot}455,$$
$$\sqrt{3}\,(x+0{\cdot}1910\,y) = \sqrt{3}\times 0{\cdot}543,$$
$$\sqrt{2}\,(x+0{\cdot}0270\,y) = \sqrt{2}\times 0{\cdot}575,$$
$$\sqrt{2}\,(x-0{\cdot}1000\,y) = \sqrt{2}\times 0{\cdot}080,$$

which give for the determination of x and y the equations

$$x+0{\cdot}1123\,y = 0{\cdot}4326,$$
$$x+0{\cdot}2381\,y = 0{\cdot}5618\,;$$

whence

$$x = +0''{\cdot}317,\ y = +1{\cdot}027, \text{ and parallax} = 9''{\cdot}04.$$

One of the results—9″·04 or 9″·06, or their mean—would, in my opinion, have to be accepted as the best value deducible from these Egress observations, were it not that we are compelled to recognise that the observations at Kerguelen were caught up with great difficulty through clouds, and that we have a later observation of Perry's, which, although called a contact, is completely separated off from the apparent contacts at the other stations, and must therefore be accepted as an observation much nearer the time of so-called real contact; and we have already pointed out that the observations in Egypt have been made when the ligament or black drop has been very largely developed. I will next take Perry's observation of contact as that of ligament well marked, and use with it the second set at Rodriguez. This is a most extreme supposition, and would not be worth consideration were it not for the fact that the observations at Thebes, which are clearly not observations of apparent contact in the same sense as

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those made at the other stations, are yet called "contacts," and are evidently the same phase as that observed at Mokattam and Suez, where it is clear that the times have been pushed to an extreme limit by the observers waiting for the ligament to assume a sufficient degree of blackness. These considerations do perhaps to some extent justify the use of Perry's observation to the exclusion of those of Sidgreaves and Goodridge, but such an exclusion is certainly not satisfactory. On this supposition we have

$$\sqrt{4}\,(x + 0{\cdot}2020\,y) = \sqrt{4} \times 0{\cdot}455,$$
$$\sqrt{3}\,(x + 0{\cdot}1910\,y) = \sqrt{3} \times 0{\cdot}543,$$
$$\sqrt{2}\,(x + 0{\cdot}0270\,y) = \sqrt{2} \times 0{\cdot}575,$$
$$\sqrt{1}\,(x - 0{\cdot}1000\,y) = \sqrt{1} \times 0{\cdot}680,$$

which lead to the equations

$$x + 0{\cdot}1335\,y = 0{\cdot}5279,$$
$$x + 0{\cdot}2128\,y = 0{\cdot}4807;$$

whence

$$x = +0''{\cdot}607,\ y = -0{\cdot}591, \text{ and parallax} = 8''{\cdot}897.$$

This value, like the value $9''{\cdot}05$, must be regarded as resting upon an extreme assumption, but in an opposite direction to the former one. But, on account of the existence of clouds at Kerguelen and the impossibility of discriminating between the kinds of contact observed, we should perhaps combine at all the stations attempts to observe real contacts without any discrimination between the finer phenomena presented. But if we look at Goodridge's observation made at $11^{\mathrm{h}}\ 1^{\mathrm{m}}\ 39^{\mathrm{s}}{\cdot}9$, we shall see that, although the limbs were only just visible through cloud, some decided connection was already established. I am hardly prepared, therefore, to consider that this observation can be fairly classed with such observations of a very early phase as that of Mrs. Campbell, which was made under the most favourable atmospheric circumstances. But as I wish to exhibit clearly the extreme limits within which the values of the solar parallax must be confined when deduced from any legitimate assumption, I will discuss these contact observations, both excluding and including the very early ones. It will be seen that the two solutions agree very closely.

The result of a discussion of the four very early observations by themselves in a separate class is not worth consideration when the circumstances under which Goodridge's observation was made are considered.

The following solution is based upon the mean of all attempts to observe real contacts, excluding only attempts at apparent contacts, which are separated at the stations at which

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both are observed by more than a minute of time from the phase used, and whose results could not therefore, under any circumstances, be admitted in the equations of condition. The following are the equations on this assumption:—

$$\begin{array}{lrl} & {}'' & \\ x+0{\cdot}2020\,y = & 0{\cdot}18 & 5 \text{ obs.} \\ x+0{\cdot}1910\,y = & -0{\cdot}04 & 5 \text{ obs.} \\ x+0{\cdot}0270\,y = & 0{\cdot}24 & 6 \text{ obs.} \\ x-0{\cdot}1000\,y = & 0{\cdot}12 & 4 \text{ obs.} \end{array}$$

The number of observations at the different stations are so nearly equal that we shall assume the weights to be equal, and we have

$$x+0{\cdot}0800\,y = 0{\cdot}1250,$$
$$x+0{\cdot}2750\,y = 0{\cdot}0725\,;$$

whence

$$x = 0''{\cdot}147,\ y = -0{\cdot}27, \text{ and parallax} = 8''{\cdot}93.$$

The residual errors are

Obs.—Comp. ″	
+0·087	Mokattam and Suez,
−0·135	Thebes,
+0·100	Rodriguez,
−0·054	Kerguelen.

There remains only a solution obtained by combining all the observations from "ligament clearly seen" to so-called "contacts," but excluding the very early observations of shadows or interference lines, a phase which is only caught by a single observer at each station, and which should therefore be excluded from our equations of condition from this consideration alone. On this supposition our equations of condition are

$$\sqrt{4}\,(x+0{\cdot}2020\,y) = \sqrt{4}\times 0{\cdot}455,$$
$$\sqrt{4}\,(x+0{\cdot}1910\,y) = \sqrt{4}\times 0{\cdot}368,$$
$$\sqrt{5}\,(x+0{\cdot}0270\,y) = \sqrt{5}\times 0{\cdot}426,$$
$$\sqrt{3}\,(x-0{\cdot}1000\,y) = \sqrt{3}\times 0{\cdot}283.$$

From these we get

$$x+0{\cdot}0879\,y = 0{\cdot}3919,$$
$$x+0{\cdot}2436\,y = 0{\cdot}4416\,;$$

whence

$$x = +0''{\cdot}364,\ y = 0{\cdot}319, \text{ and parallax} = 8''{\cdot}979.$$

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The residual errors are

Obs.—Comp. ″	
+ 0·027	Mokattam and Suez,
− 0·057	Thebes,
+ 0·053	Rodriguez,
− 0·049	Kerguelen.

These residual errors are very small, and this appears to be the only satisfactory way of obtaining any good *independent* value from these Egress observations. The value thus found is larger than we have reason to believe it should be, but the probable error of the determination is considerable. From these Egress observations, then we have,

$$\pi = 8''{\cdot}979 \pm 0{\cdot}279 \times e,$$

where e has a similar meaning to that in the Ingress observations.

It appears to me that these Egress observations cannot be satisfied in any way by a value less than 8″·88, and that the value 8″·98 is that which is derived from them by the most satisfactory *à priori* assumption of corresponding observations. Accepting, therefore, for the Ingress 8″·860 ± 0·136 e, and for the Egress 8″·979 ± 0·279 e, we have for the mean solar parallax

$$8''{\cdot}884 \pm 0{\cdot}123 \times e \tag{5}$$

as the value deducible from these contact observations, a value which I believe absolutely negatives any value smaller than 8″·84 or larger than 8″·93.

My own earlier determinations are

	″		
(1) From observations of *Mars* in 1862	8·94	Published	1863.
(2) From parallactic inequality in Earth's motion A value of small weight from its depending on the assumed value of the Moon's mass.	8·91	„	1867.
(3) From parallactic inequality in Moon's motion Deduced from 2075 observations near the times of maximum effect.	8·85	„	1867.
(4) From a rediscussion of Transit of *Venus* Observations, 1769	8·91	„	1868.

These five determinations give me

$$\pi = 8''{\cdot}897 \pm 0''{\cdot}020.$$

Besides these I give for comparison with my results the following:—

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Dr. Winnecke, from the *Mars* observations in 1862, deduced a value 8″·96. This was published in 1863.

Mr. Newcomb's discussion of the *Mars* observations in 1862, published in 1867, gave 8″·855. The smaller value deduced from these *Mars* observations by Mr. Newcomb depends chiefly on the use of the Santiago observations, where the inclination of the wire used was not determined. Mr. Newcomb has attempted to eliminate the errors thus introduced, but I think that the use of these Santiago observations is, under the circumstances, inadmissible.

Hansen's value, deduced from the parallactic inequality in the Moon's motion, is 8″·916. This value was first published in 1867. Hansen had intimated several years before that the parallactic inequality required an increase, but it is doubtful whether he was prepared at that time to throw the whole of this required increase upon an error in the assumed value of the solar parallax. There are also the determinations from the velocity of light which have lately been repeated by M. Cornu, and which, with the accepted value of the coefficient of aberration, lead to a value 8″·86. And there are the classical investigations of Le Verrier, in which, starting with an erroneous value of the ratio of the Sun's mass to that of the Earth deduced from Encke's value of the solar parallax, he not only showed from the theories of the planets themselves that the assumed value of the solar parallax was incorrect, but deduced a value 8″·88, which is entitled to great weight, and is in most satisfactory agreement with the values deduced more directly from observation. If we assume $\pi = 8''\cdot 89$, the correction which this assumed value requires must be very small. This will correspond to a distance of the Earth from the Sun equal to 91,940,000 miles.

Royal Observatory, Cape of Good Hope,
1878, *January* 18.

On Certain Groups of Stars with Common Proper Motions.

By Prof. T. H. Safford.

It has been long noticed by those who have given attention to the subject of proper motion that there are many groups of stars which seem to have a common proper motion. This tendency has been called star-drift by Mr. Proctor; and the special instance which he has brought prominently forward is that of the group of five stars in *Ursa Major*, β, γ, δ, ϵ, ζ. The motion of these stars is nearly in the same direction, nearly opposed to that which results from the solar motion, but varies in amount from about 0″·09 to 0″·15 yearly.